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I.

ON SUPPOSED DISEASES OF THE SPINE, AND THE ILL EFFECTS OF A CONTINUED RECUMBENT POSITION.

By J. SWAN, Esq.

WHEN pain in the course of the spine has been complained of, and particularly if there has been any tenderness on pressure, it has been determined that inflammation existed in the ligaments, or some other parts equally affecting the integrity of the spinal column. The opinion founded on this symptom alone, has sometimes been verified by subsequent changes, but has too frequently proved erroneous, even when pain, tingling, and other nervous affections, have been present in the extremities.

If there be no disease in the seat of pain, nor in the tender part, from what does the altered sensibility proceed? When it is merely in the skin, the different parts of this, by being supplied with the spinal nerves, may have the morbid excitement conveyed to it through the sympathetic nerve. Thus, in diseases of the liver, pain is felt in the shoulder, from the communications between the hepatic plexus and the phrenic nerve. So each part of the sympathetic being also in connexion with each spinal nerve, may, according to the viscus affected, excite a morbid sensibility in

that spinal nerve communicating with it, and appear in the termination of this in the skin. The same tenderness, from similar causes, may be present in the integuments of the chest and abdomen. But in some people the pain may be in the muscles, or the nerves supplying these, and arise from the same connexions with the sympathetic, but with a modified cause; for the same nerves can convey one impression to the muscular parts and another to the sentient, as may be instanced in the former by the use of the nux vomica, and in the latter by opium. The origins of the morbid excitement may be seated in the stomach itself; it may be also in the liver, intestines, kidneys, the bladder and uterus, and sometimes in the viscera of the chest. It is hardly necessary to mention that pains may be excited from the connexions of similar nerves, as between those of the uterus and limbs, and be felt in the extremities of the different branches of the nerves, in the same way as pain is experienced in the penis in diseases of the bladder.

When pain in the spine is complained of, and there is tenderness on pressure, should a constant recumbent position be had recourse to? If there be actual disease in any part of the spine, such as a projection of the spinous processes and thickening of the surrounding

parts, no one can doubt the necessity or utility of keeping it in this manner perfectly quiet: but mischief may be going on in the anterior part of the spine, and no decisive diagnostic symptoms be present. Is, therefore, so formidable a disease as that affecting the vertebral column and the parts connected with it, to be risked, because no external appearances of disease are present? If there be an equal possibility that the cause of the pain, &c., is not seated in the spine itself, should the recumbent position and a state of continued rest be enjoined, or merely because a sense of weariness or pain is to be obviated? If the confinement were to be of only a short duration, it could not be altogether objected to, but, in a modified degree, might co-operate with the other medical treatment in removing the irritating cause. When the disease is not in the spine itself, the pain and tenderness vary, both as to their seat and intensity, with the state of the digestive organs. But if there be considerable doubt about the cause of the symptoms, the recumbent position may be so used that the patient shall keep the spine sufficiently quiet, and yet not remain continually in this state. If a patient be kept in an entire state of rest in the recumbent position, and a beneficial result be not obtained within a moderate space of time—say after the lapse of some weeks—the consequences peculiar to this treatment must ensue. It will be asked, what is to be decided from so apparently harmless a remedy, or how can any mischief be produced? The circulation in the head must be increased by a position which favors the ascent and retards the descent of the blood; and by this, after a time, every

part within the skull is encroached upon, the furrows on its inner table are deepened and extended in every possible manner, and we cannot suppose that the bone shall be thus acted upon by the increased circulation, and the cerebral mass remain uninjured? Undue pressure from diseases may produce great changes and a diminution of the brain, and I have seen this affected by long-continued determination of blood to it, and believe this may also be induced by disease, too much indulgence in the recumbent position, or too stimulating a diet. It is known that the muscles of the spine, and even the bones themselves, have their strength diminished by inaction, and consequently, after a long use of the recumbent position, are unable to hold the trunk erect without the greatest exertion. But the change in the circulation in the head, arising from a long-continued recumbent position, leaves the brain unsupported by a sufficient quantity of blood, and produces most uncomfortable symptoms: doubtless the change in the position of the other viscera creates some uneasy sensations, but in a very inferior degree. After an attack of paraplegia, from an affection of the brain, should there be an indentation larger than natural between two spinous processes, or deformity of one of these, and therefore it be supposed that the symptoms proceed from a diseased state, and a recumbent position strictly adopted, if the patient could walk before this treatment was begun, it will be found, partly from the oppression the brain has been subjected to, he has entirely lost the command over the muscles, and that he cannot return to his former state until a long time has elapsed.

If the cerebral mass shall have suffered a diminution, can it be restored, or must it be supplied with the same inordinate quantity of blood continually, in order to put it in a sufficient state of tension? The brain and the circulation of its blood may be brought to their original condition by sufficient care and attention, but not without the greatest resolution; for besides the uncomfortable sensations produced in the brain itself, the difficulty of keeping the body erect is not a little increased by the pain excited in various parts from a change of position.

If the brain suffer, as I believe it does, ought a recumbent position to be used without the greatest caution? It is not in supposed diseases of the spine that the brain becomes oppressed, but frequently from slight indisposition and indolence. Thus a person feels unwell, and lies long in bed; he becomes enervated; and this position, if continued, increases his complaints: he soon becomes weary when up, and rests again and again, until such a change is effected in the brain that he cannot, without the greatest difficulty, return to his former state. And if a change of so much importance can be thus effected, is it not of sufficient moment to pause before such a state is produced as time can with difficulty obviate? And ought not this to be a subject of serious consideration, not only to the medical profession, but to every class, both as respects their comfort, their health, and, above all, their mental faculties. Too much vascular action, whether produced by overfeeding or indolence, may injure the mental faculties, and Shakspeare has well remarked,—

"Fat paunches have lean pates, and dainty bits
Make rich the ribs, but banker out the wits."

Lond. Med. Gaz.

II.

REMARKS ON THE STAINS PRODUCED BY THE IMPROPER USE OF NITRATE OF SILVER IN SOME DISEASES OF THE EYE.

A FEW months ago, a paper appeared in the Dublin Hospital Reports, by Dr. Jacob and Mr. Lawrence, in which note was taken of the stains which sometimes remain permanently on the conjunctiva and cornea, from the application of Nitrate of Silver to those organs. The following remarks of Mr. Hunt, Assistant Surgeon to the Manchester Eye Infirmary, were designed to counteract the effect of the above-mentioned Report in limiting too much the use of that powerful curative agent in some forms of ophthalmic disease. It will not be found uninteresting to the practitioner.

In most ophthalmic institutions, by far the greater number of cases of disease consist of opacities and ulcers of the cornea, and there is perhaps no part of the country in which so great a proportion of these affections depends upon strumous inflammation, as in Manchester and its neighborhood. Among several hundreds of these diseases, in which the nitrate of silver has been used, both by applying it in substance to ulcers of the cornea, and to the conjunctiva of the lid, as well as in solution, not a single instance has occurred of the livid, olive, or, as it may be called, bronze stain, to which Dr. Jacob and Mr. Lawrence have alluded. The experience of several friends is fully accordant with the above statement, and particularly that of Mr. Barton, the present senior surgeon of the institution, who has continued to employ the same remedy during a much longer period. I have certainly seen cases in which this dis-

coloration has occurred; one instance in particular, where the stain which affected the conjunctiva of one eye only, was so deep that the difference between the eyes was perceptible at a very considerable distance. But here, as well as in several other cases which have come under my notice, there was no difficulty in accounting for the change of color, as a strong solution of nitrate of silver had been applied for many months without attention to those precautions which are always necessary during its employment. Let not, then, so valuable a local application be discarded because, like all other good things, it is liable to abuse. For, indeed, the remark of Mr. Lawrence, with which Dr. Jacob apparently coincides, viz., "that he does not see how lunar caustic, which has been so much recommended, is to act upon the diseased eye beneficially," has a great tendency, when coming from such authorities, to throw into disrepute this useful remedy.

Dr. Jacob himself states, that he has not observed that the application of even a strong solution, for a fortnight or three weeks, will produce the effect, but he believes its continuance for six weeks, or two months, will do so. Is not this stating, in other words, that when properly used the nitrate of silver does not cause the stain, and *vice versa*? The remark, also, of the beneficial influence of the nitrate of silver in the hands of such a man as Scarpa, when closely examined, resolves itself into the general observation, that a surgeon can only employ a local remedy, or plan of treatment, with advantage, when he understands the description of cases to which it is applicable. That lunar caustic

does act upon the eye beneficially, very beneficially, in many instances, is a fact so firmly impressed upon my mind, by the results of the practice of others, as well as by the numerous trials of its efficacy which I have made, that not even the authority of Mr. Lawrence or Dr. Jacob can induce me to think otherwise, whilst I have every day before my observation the great advantages derived from its use. To point out all those diseases of the eye in which the nitrate of silver is of service, and the regulations necessary for its proper management, with any moderate degree of accuracy, would lead me farther than is my intention at present: I shall be contented with briefly stating, that in cases of strumous inflammation of the conjunctiva, whether accompanied by opacity of the cornea or not, where the intolerance of light was so great as to render the least opening of the eyelids very annoying to the patient, I have known a single application of the nitrate, in substance, to the conjunctiva of the lid, effect so much relief by the next day, even when unaccompanied by constitutional treatment, that the patient has been enabled, in a moderate light, to keep the eye half open, without any uneasiness being produced. Not that I underrate the value of internal medicines in such diseases, but I have stated the result of the application, when unaided by any other means, with the view of proving its utility in a manner which can admit of no objections, founded on the use of other remedies at the same time. When the state of disease already mentioned is combined with an ulcer of the cornea, which seems to resist the usual treatment, or one from which a slough has recently

separated, leaving a surface, in which the ulcerative process seems still going forwards, the application of a fine point of lunar caustic to the centre of the ulcer, which has been recommended by so many practical surgeons, has, according to my own observation, fully justified the reputation it has acquired. Nor have I ever witnessed the indelible dark speck, or black opacity of the cornea, mentioned by Dr. Jacob, as resulting from the employment of the caustic, either in substance or solution. The only opacities of the cornea, caused by the healing of ulcers, however differing from each other in the extent of injured structure, appear to me divisible into those where the ulcerative action has been confined to the conjunctival covering of the cornea, and those where it has affected, not only this covering, but also penetrated into the proper laminar texture of this tunic. In the former, we have a semi-transparent opacity, of a bluish or greyish white, which eventually disappears; whilst in the latter, the central part of the opacity is of a different shade of color, more nearly approaching the yellowish white of ivory, and which never again becomes translucent. The permanent dark spots, found both in the conjunctiva and cornea, produced by the presence of particles of gunpowder, charcoal, and oxide of iron, in these tunics, do not appear to bear a very strict analogy to the specks in question, in consequence of the different circumstances under which they are introduced into these textures. When the caustic is properly applied to an ulcer, no part of its substance is allowed to remain in the cavity except that portion which is dissolved by the action of the fluid, lubricating the ulcerated sur-

face; and however satisfactorily the application may act, in stimulating this surface, so as to check the ulcerative absorption, before existing, and encourage the deposit of lymph, or that fluid, whatever it is, which accomplishes the filling up of the ulcer, still this is a work of time, and the cavity, during this period, is continually washed out by the secretion of tears, which becomes more plentiful immediately upon the application of the remedy.

The nitrate of silver, also, when acted upon by the secretion, forms instantly a white precipitate. If this precipitate should remain in the ulcer, which is never dry, but continually covered by fluid until it heals, this fluid will prevent exposure of the precipitate to the atmosphere; which, if I understand, Dr. Jacob adduces as the cause of its becoming black, or brown, and, consequently, will also prevent the formation of the indelible dark speck so often before alluded to; whereas, in the case of a grain of gunpowder, or any other of the particles before spoken of, being forcibly driven into the cornea, this tunic is lacerated, and if the eye was in a healthy state previously, this laceration is soon united by adhesion, so as to cover the foreign body, and protect it from the action of the secretion which continually moistens the eye. Besides the dark spots formed by foreign bodies imbedded in the conjunctiva and cornea, the only others which have occurred to me are those caused by prolapse of the iris, or choroid coat, owing either to wounds or ulcers, and those cases in which a small portion of the iris remains adherent in the centre of an opacity, where an ulcer formerly existed.

Another instance of the utility of the caustic, is in those cases of

chronic purulent ophthalmia, so frequently met with in men who have been formerly on foreign service in the army, and generally known by the name of Egyptian ophthalmia, where the conjunctiva of the lids is so much altered by disease, as to present a granular surface. These cases, generally so obstinate in their nature, are more improved by the repeated use of the nitrate than by any other plan I have witnessed. In one at present under treatment, the patient, who had for many months required a guide to lead him, was so much relieved by the granulations being freely dressed with the caustic three times in the week, that, in the course of a month, he was enabled to walk alone through the streets; and up to the present time, an interval of more than twelve months, the improvement continues to proceed, notwithstanding he has suffered occasional relapses, owing to irregular attendance. Although, in prolapse of the iris, and many other diseases of the eye, this application is equally serviceable, I trust enough has been already stated to assist in confirming those who have been in the habit of employing this valuable remedy, and to induce those who have not, to give it a fair trial.

III.

OF MALADIES WHICH MAY ARISE FROM RIGORS, OR ELSE FROM COLD, ACTING ON THE INTERIOR OF THE HUMAN FRAME.

For the Boston Medical and Surgical Journal.

DISTINGUISHED men often appear, who, besides their greater labors forming the principal sources of their fame, leave behind them frag-

ments of instruction on secondary subjects. Dr. Kirkland (the father), an able practitioner in the last century, seems to have been one of this description,—as might be proved, were this the proper moment.

It is worth, however, commenting here on one particular sentence of Dr. Kirkland's, delivered by him in a transient manner; since it may lead the attention of medical practitioners to what is believed to be the cause occasionally of serious mischief, and sometimes even of death itself, both in cold countries, and also in countries subject to fevers in which rigors or shiverings of a *certain kind* sometimes occur; namely, the *long-continued operation upon the human frame*, either of external cold or of these internal rigors. The sentence from Dr. Kirkland alluded to above, and leading to this remark, is the following; which it is proposed should be taken as insulated and independent of other matter.

“So far as my observation goes (says this writer), where the *nerves* appear to be chiefly affected, the fate of the patient depends upon the *manner of attack*; because where the *rigor is excessive*, few only recover,—owing, perhaps, to an extreme irritability of a particular kind, and to the injury [which] such a state of the nerves receives, when affected by any *external cause which greatly lessens their energy*.” So far Dr. Kirkland—and now follows our commentary.

It is admitted that the sentence here quoted seems to have principal reference to diseases originating from *internal rigors*; nevertheless, under cover of it, we shall refer also to injuries produced by *exterior cold*; as each affection must act upon the other, as well as have se-

veral points between them in common. Far be it from us to assign to them, on this account, a joint place in our nosologies; as they are too unsteady and arbitrary in their symptoms to admit of any such classification. Our remarks are of a more limited and restricted nature.*

We shall now relate a *few cases* bearing upon one of the points which we have in view; leaving the *other cases*, bearing on the other point, for others to notice: and as we proceed, we shall farther explain our meaning.

We begin, then, with our *first* set of cases.

In the first instance, a substantial farmer, of connections well known to the public, and living in one of the towns of Maine, having suffered from heat in the month of May, dressed himself in his summer clothing; and though the weather afterwards became cold, he neglected to return to his winter dress. It is not known that he felt any particular *pain* at the time (for we often take a common cold without feeling *pain* at the moment); but he contracted a peculiar set of symptoms, which were attributed to the above circumstance, and were not to be removed by the remedies *ostensibly* called for; and he was carried off by them in a few months.

A *second* case to which we shall refer, was abundantly more marked. An eminent lawyer, whose business had called him to the sea-side, re-

turned home (about 18 miles) in a severe winter's evening. Though pressed by his family to take prompt and suitable measures for recovering himself, he chose to wait more leisurely than was wished, for his evening refreshments; but, in the mean time, he manifested strong marks of being penetrated with cold—being unable, even by the fire-side, to keep his hands and arms from being drawn to and fro, from right to left, in a convulsed or paralytic manner. Fevers not being uncommon at the time, it was hoped that this disease (for it soon became a severe one) would prove only one of the fevers then prevailing; but, though skilfully treated, he languished a certain number of days, and then died. Here actual death, we perceive, followed in *two* cases.

The *third* case has not terminated in death, but it was attended by such *specific* evidence of its origin, as to leave no reasonable doubt of its *cause*. The son of a very distinguished French General, passing, late in the fall of the year 1828, by Frenchman's Bay (so called) in Maine, determined, from national feelings, to take a sketch of this bay with his pencil; and seated himself, for this purpose, quietly on a cold rock, in cold weather. An English naval officer, his travelling companion, having in vain remonstrated on the disproportion between the object in view and the risk he ran, left him for a time to himself; so that an hour perhaps was consumed in the above operation. Notwithstanding great appearances of previous health, activity, and strength, this young person soon found himself very seriously indisposed, and became confined, with little exception, to his chamber for several weeks, with a

* It is to be observed that our subject has nothing to do, under any shape, with cases where death occurs from external cold, acting *without slow preliminary symptoms*; for here the parties are said to be *frozen to death*: nor does it include cases where the surface of the body or the extremities alone are injured; for here the patient is merely said to be *frost-bitten*.

most obstinate difficulty in passing his urine, besides having other connected complaints; though his health, in other respects, was less affected than might have been expected. He had good medical advice soon after his first attack; and, subsequently, he obtained the opinions of several eminent practitioners in some of the first cities in the United States; but he long endured this complaint in a formidable shape, even if he has now recovered from it.

The notice of a *fourth* case of the kind in question is now to follow, being that of the celebrated English Minister, Mr. George Canning; who, having been previously unwell, became seriously chilled, or affected in his nerves, or both, in consequence of his bearing part in the peculiarly long and slow-paced funeral procession of the King's brother, the Duke of York; and died in consequence of this complicated attack.

It is plain that *these four* cases bear a pointed reference to the action of *exterior cold*; but it is hoped that there are various readers of these remarks, who will be able to furnish *another set of cases*, equally interesting, where the primary cause of the maladies which ensued, will be found in *rigors*: we mean such rigors as abound in some of the febrile diseases common to the larger part of the United States, and such as are above described.

Upon the statements, however, already presented, a query or two may be founded; namely, 1st, as to the *nature of the evil* produced; and, 2d, as to the *mode of proceeding* to be used in the *first instance* respecting it. What will be offered under these two heads, will bear a closer relation, perhaps, to each of the two sets of cases in view, than might at first be suspected.

1st. As to the *nature of the evil* produced, we cannot yet hope, with regard to the cases arising from exterior cold, to collect much aid from the observations of others, nor yet from dissections; but we may say something ourselves on the subject *hypothetically*, and by means of hints and conjecture.

First, then, we remark, that the yolk and the albumen of the egg of a bird, being once coagulated by heat, each remains thus fixed forever; fluidity being incapable of being restored to *either* of these substances. Hence we perceive that a change of temperament having once operated in certain cases, the result may be indelible. Next; the flexibility, the firmness, the motions, the porousness, &c., of certain important organs and substances, may sometimes be essentially affected in cases of this sort. Again; certain fluids of the system may become injuriously and irrecoverably *intermixed*, by severe agitation; or, in other words, there may, by this means, be an *error loci*, of a serious nature, occasioned in certain instances. It is needless farther to multiply hypotheses, after having offered these suggestions. But it is right for a theorist to bear in mind (such is the versatility of nature) that various *insects*, as far as cold is concerned, may bear an immense amount of cold, without being injured as to their fluids, solids, or organs.*

But to proceed to our second general remark: we notice, that various evils here referred to, are often obviously open to *two* early measures for their relief; namely, first, that of withdrawing the patient

* The case of animals in a *dormant* or *torpid* state, differs again from that of the particular insects above mentioned, which seem to defy all power of frost.

from the *cause* of the injury, where it is not internal; and next, that of applying warm and well-known stimulants to the body externally and internally—observing, as with frost-bitten patients, that there nevertheless may possibly be some danger of applying our antidotes too precipitately. I say nothing, moreover, of what happens in internal rigors as above described, when an *epidemic* prevails; for here something may act contrary to rules belonging to common cases, there seeming to be that in an epidemic, which (in the phrase of Sydenham) is “an entire subversion of the [usual] rules of the animal economy.”

As to the *sequel* of the proceedings to be adopted in these several cases, it is plain (such is the analogy between the two branches of our subject) that each case must in the *end* be treated as its own particular symptoms shall suggest.

Here our commentary must terminate; but in reviewing it, we have the satisfaction of being persuaded, that as the subject is now brought into view, remarks will multiply in the hands of others; and that in the hands of none is mischief probably likely to ensue, notwithstanding (thanks to the sagacity of Dr. Kirkland) this new branch of theory and of practice in medicine, is now first brought into notice.

What has been said here by the writer of this paper in his own person, is under an avowed sense of its imperfections; but

Valeat quantum valere potest.

P. S.—Reference might perhaps have been made, in the body of this paper, to the last illness of the late Dr. GORHAM, of Boston; which began by exposure to con-

siderable cold, and was aggravated both by his excess of confidence in his own constitution, and by his persuasion that he had passed with impunity through various similar scenes before. Some of the first physicians in Boston, who in the end were consulted, and whose opinions have been in some degree made public, do not seem to have been altogether disposed to have classed his case, where we perhaps should have put it. But had the attack appeared in a still more *severe* shape than that which presented itself, their opinions, also, might possibly have been modified; since it is plain that the evil might have proceeded considerably farther than it did, and have increased in its approximation to the amount described in the paper above, without going to the length of destroying the patient by producing *immediate* death (that is, *primo ictu*); and the question here we see is as to a degree of measure and amount, and not to an *extreme* case. *

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POISEUILLE'S RESEARCHES ON THE VENOUS CIRCULATION.

IN the celebrated experiment of Barry, performed with the view of determining the mechanical cause of the circulation, the jugular vein of a horse having been carefully divided, a glass tube was introduced by its two extremities into the two cut ends successively, so that the circulation took place through the tube itself for a distance of several inches. The central part of the tube thus employed was of much larger diameter than the extremities, forming

a sort of bulb, capable of containing a considerable amount of liquid. By carefully examining this part, which was left wholly exposed, Dr. Barry satisfied himself, and several individuals present, that the blood flowed toward the chest at every inspiration, and retreated or remained stationary during expiration. As the circumstances of the experiment were peculiarly favorable, and the observation steadily continued for a considerable time, there could be little doubt as to its accuracy; it appeared evident, therefore, that the moving power of the blood, in this case, was the pressure of the atmosphere on the veins, exerted during the period when a vacuum was formed within the chest by the depression of the diaphragm. From this and other experiments, the details of which may be seen in his work, Dr. Barry was led to adopt the following conclusions:—1st, that the blood which runs contrary to its own gravity, arrives at the heart only during inspiration; 2dly, that the power which impels it at this period through the veins, is atmospheric pressure; 3dly, that as this power can be applied to the blood of the veins only at the moment of inspiration, this fluid must move with a velocity which is to that of the blood moving through the arteries, as the time of a whole respiration is to that of an inspiration only.

It is to be observed, that neither in these propositions, nor in those afterward advanced, did Barry go to the length of asserting that atmospheric pressure was the sole cause of the venous circulation. That he

believed it to be the principal one, and to extend throughout the venous system, is evident from the calculation above made, which could be exact only so far as this was the case, and equally so from the manner in which he accounts for the motion of the lymph and chyle; which he says "are sucked up towards the chest, through the direct communications which their vessels have with the subclavian and other veins." (*Researches*, p. 37.) He afterwards assigns two other causes of the circulation in the veins—namely, the force of the heart's action on all these vessels, and the influence of gravity on those which descend; but adds that "the pressure of the atmosphere is by far the most intense in its degree, the most constant in its influence, and the most unvarying in its amount. It is that without which the circulation could not be maintained beyond a few moments." P. 57.

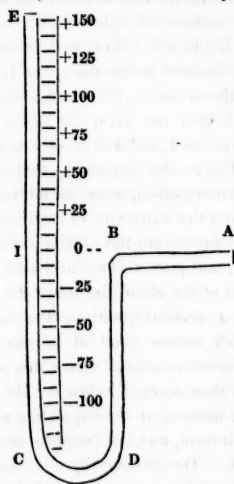
The objections which have been made to the views above stated, are probably, for the most part, familiar to our readers. They are principally the following:—1. That the partial vacuum produced in the chest by inspiration, being immediately supplied by air from without, cannot act with any force upon a denser fluid in an opposite direction. 2. That the veins being flexible tubes, the pressure of the atmosphere would tend to approximate their parietes, rather than to force a fluid through their cavities. 3. That the venous circulation is not peculiar to animals which perform the process of respiration by means of lungs; that in

fishes, which breathe by gills, and in the fœtus, independently of respiration, the venous circulation goes on with the same vigor as in man. The second of these objections had been anticipated by Barry (p. 47), and the last was distinctly stated in the very able and cautious report of Cuvier and Dumeril to the Academy. But while the analogy of other physical and vital processes was summoned to show that what Barry had advanced could not be true, no one appears to have been prepared to assert that it was actually false. His great argument, the experiment, remained still unanswered. In order to combat this, it was necessary to show one of two things,—that the observer had deceived himself and the illustrious men present, in supposing a phenomenon to take place which in fact did not occur; or, 2dly, that admitting its truth, it was susceptible of a different and more satisfactory explanation. Such is the inherent difficulty, however, of proving a negative proposition, and such the apparently increasing diffidence in the results of experiments on animals, that the inquiry appears to have been feebly, if at all, prosecuted, and the subject to have virtually remained where Barry and the French Academy had left it.

Dr. Poiseuille, of whose ingenious researches into the arterial circulation we gave an account in a former number, and who, to uncommon dexterity in experimenting, appears to unite a spirit of rigorous calculation not always found among physiologists, has lately turned his attention to this subject; and while he con-

firms the accuracy of the observation made by Barry, has added others by which the conclusions of that philosopher must be considerably modified: in other words, M. Poiseuille has arrived at the conclusion, "that the suction due to the dilatation of the chest and of the right side of the heart, is not the principal cause of the movement of the venous blood; that the operation of this force is limited to the large venous trunks, which discharging themselves in the chest, less resistance is offered to the smaller branches: the force in question is therefore an auxiliary, but nothing more."*

The mechanism adopted by M. Poiseuille in conducting his experiments, though considerably different from Dr. Barry's, was sufficiently simple, and can be readily comprehended by means of the annexed plate.



* Vide memoir read to the Institute, Sept. 30, 1830.

It consisted of a glass tube, curved as represented in the figure, and having its extremity, A, fitted to receive an additional tube, of a diameter corresponding to the vein which was made the subject of the experiment. The portion from A to I was filled with a solution of subcarb. sodæ, which salt has the property, when mixed with the blood, of preventing its coagulation. Along the great branch E I D, was placed a scale perpendicular to the line A I, provided with a plumb line to secure its vertical position. The scale was in the original divided by millimetres, for which we have substituted lines, or tenths of an inch. The zero of the scale was at I. The manner of using this instrument is easily seen. A tube of proper diameter being added at A, and introduced into a vein towards the heart, if a vacuum take place in the vessel, the liquid will follow, and its surface will descend below the point I. If, on the contrary, the blood is thrown back into the vein, the liquid will rise above I, and will move toward E.

The results obtained by employing this mechanism, were as follows:—When the extremity of the tube was introduced into the jugular vein of a dog, and passed down to within four lines of the chest, the long tube having a vertical position, the liquid, which before stood at I, was first observed to ascend above this point, and then descend below it; the former movement corresponding to the expiration, and the latter to inspiration. Designating by + and — these different states, the height attained at the first inspiration was

—36; at the expiration following, +34; then —28 and +24; and these differences in elevation continued to succeed each other for the space of ten minutes. The same experiment repeated on the opposite jugular, gave the same results.

In another animal in whom the right jugular was operated on, the additional piece employed was 32 lines in length; and the vessel being opened near the chest, the instrument reached, in that cavity, nearly to the right auricle. In this experiment, the degrees obtained in the inspirations were —32 —48 —60 —52; and in the expirations, +48 +52 +92 +80.

In order to leave no doubt that the preceding phenomena were produced by the rarefaction of the air in the chest, the experiment was varied as follows:—A metallic tube, provided with a stopcock, being introduced into the trachea, and the instrument being applied to the vein, the elevations were nearly as before, —32 +36, —30 +32, —24 +26, —32 +36. The stopcock was then closed immediately after an inspiration; the liquid immediately descended, and gave, successively, —16, —24, —30, —32, —40, —48, —56, —60. It is to be observed, that the longer the time that elapsed after the air was excluded from the chest, the more violent were the efforts at inspiration, the greater the expansion of the chest and the pressure of the external air; which precisely corresponded to the indications given by the instrument. On permitting the air to re-enter the chest, the liquid again rose, present-

ing the elevations -32 , $+28$, -30 , $+32$, &c., as before: the stopcock was then closed, not after an expiration, as at first, but when the animal had inspired, and the degrees presented were $+16$, $+20$, $+28$, $+38$, $+41$, $+46$, $+48$; a result which might of course be expected to occur, from the efforts made to expel the air, and the consequent increased pressure, from within outward, exerted directly on the venous trunks. Similar phenomena to these also occurred when the subclavian and the vena cava inferior, at their entrance into the chest, were made the subject of experiment; thus confirming the opinions of Barry so far as to show, that the chest, at the moment of inspiration, acts on the blood in the large venous trunks which it contains, and thus becomes a cause of the movement of this fluid.

Having repeated the experiment on the jugular vein, under circumstances apparently similar to those above stated, M. Poiseuille remarked that while the inspirations produced a descent of the fluid similar to what had previously been noticed—namely, to -28 , -32 , -36 , -30 , -34 , successively—the corresponding expirations produced -2 , -6 , -4 , -2 , $+1$. On dissecting the vein which presented this result, the reason of the difference was perceived. It was found that a valve had intervened between the extremity of the instrument and the thorax, which had impeded, without wholly preventing, the reflux of the blood. This explanation was subsequently confirmed by another experiment, in which the extremity of

the tube, placed at 12 lines distance from the thorax, showed scarce any elevation above zero, but, on being urged somewhat farther, immediately presented corresponding degrees of elevation and depression.

Having thus ascertained the effect of the suction power of the chest on the large vessels, M. Poiseuille proceeded to inquire to what distance from the thorax itself this influence extends. Having opened the jugular vein in a dog, eight lines below the point where it receives the facial, he directed the extremity towards the chest, so as to make the intervening space about 5 1-2 inches. The liquid remained at zero for seven minutes; then, the respiration being more violent, oscillated once from -2 to $+1$, and again became stationary at 0: the moveable piece being then exchanged for one of greater length, so as to make the distance two inches, the liquid descended to -32 , then rose to -6 , and continued to oscillate between these two points during the respiration which succeeded; the greater elevation of the liquid being, as in the former instance, prevented by the valves.

In a subsequent experiment, the extremity of the tube, introduced into the brachial vein, was placed within three inches distance from the chest, the intervening valve of the subclavian having been ruptured so as to permit the reflux of the blood. In this case, the liquid constantly remaining above 0, oscillated from 48 to 60, and from 32 to 40. The respiration being suspended at the end of an inspiration, the liquid

rose to 80, then descended to 24, rose to 76, and descended to 26: the air being allowed to enter the chest, and the respiration again suspended after an expiration, the level of the liquid varied, during the succeeding efforts of the animal, from 8 to 24 above 0.

In a third experiment, in which the brachial vein was opened, the tube was passed to within four inches of the chest. The liquid remained permanently at 0, even when the respiration was most violent. Finally, in a fourth trial, in which the distance of the instrument from the chest was nine inches, the surface of the liquid remained equally immovable, although violent efforts to expand and compress the chest, were produced by closing the trachea successively when this was distended with air, and after it had been emptied by expiration.

Such are only a part of the facts on which M. Poiseuille has founded his modification of Dr. Barry's theory. That this theory required modification, and that the influence attributed by Barry to the atmospheric pressure was unduly great, has been, as we have already observed, a very general impression; but the merit certainly belongs to M. Poiseuille of setting the subject in a clearer light, and of assigning, with a very considerable degree of exactness, the limits within which this influence is exerted. Some farther researches by the same author, in this important department of physiological inquiry, will be considered at some length in a future number.

MURRAY'S CURE FOR CONSUMPTION.

OUR notice of Mr. Murray's new mode of treating this disease, was copied into the N. Y. Commercial Advertiser, and the intelligence came with such deep interest to persons in every quarter of the country, that very numerous have been the inquiries sent us for further information and the work of Mr. M.—In reply to these we published, a few weeks ago, some directions, sufficiently minute, for using the nitric acid in this disease.—We are gratified to notice, in the newspaper before mentioned, the following note, with the Editor's testimony to the veracity of its author.

"Cure for Consumption.—We give place to the following communication with much pleasure. Mr. E. White, the gentleman whose signature is attached to it, is well known to the community, and the utmost reliance can be placed upon any statement made by him. The discovery, if, after being fairly tested, it shall be found efficacious in other instances, is truly a valuable one, and is well worthy of the attention of medical men.

To the Editors of the Commercial Advertiser.

Seeing it stated in your paper, a few weeks since, that inhaling the fumes of nitric acid had been found to cure the consumption, at my suggestion and request, a worthy and intelligent man who has been for two years past in my employ, has, within the last eight days, given it a trial. As no directions accompanied your notice, the following course was adopted:—Under a handkerchief, one end of which rested on the head, the other left to fall down over the breast, he held a glass tumbler, hav-

ing in it a teaspoonful of the acid, and breathed the fumes, thus prevented by the handkerchief from escaping, about half an hour at a time, three times per day. The effect has been to relieve him *entirely* from an obstinate cough of many years standing, and which, for the last two months, had been accompanied with all the symptoms of a confirmed and rapid consumption; all which have, according to *present appearances*, disappeared with the cough. How permanent this relief may prove, time must show.—That others, laboring under the like affection, may be put in early possession of the above facts, and with a hope that relief may be found therefrom, this communication is made by request of the person above referred to. Yours, &c.

E. WHITE.

N. B.—The gas can only be breathed at the mouth—and to prevent the eyes being affected by it, keep them closed."

Iron in Chorea.—Dr. Elliotson, of St. Thomas's Hospital, London, says he has *never failed* of curing chorea by the subcarbonate of iron, if duly persevered in. Two drachms were given twice a day, in ordinary cases. The good effects sometimes began to appear in two or three weeks, and a continuation of the remedy three months was sometimes necessary. We expect too rapid cures, in this and many other diseases;—too little perseverance is a common failing with both patients and physicians.

Effects of Haste in increasing and diminishing Davy's Corpulence.—Such was his great celebrity at this period of his career, that persons of the highest rank contended

for the honor of his company at dinner, and he did not possess sufficient resolution to resist the gratification thus afforded, although it generally happened that his pursuits in the laboratory were not suspended until the appointed dinner-hour had passed. On his return in the evening, he resumed his chemical labors, and commonly continued them till three or four o'clock in the morning; and yet the servants of the establishment not unfrequently found that he had risen before them. The greatest of all his wants was time, and the expedients by which he economised it, often placed him in very ridiculous positions, and gave rise to habits of the most eccentric description: driven to an extremity, he would in haste put on fresh linen, without removing that which was underneath; and, singular as the fact may appear, he has been known, after the fashion of the grave-digger in Hamlet, to wear no less than five shirts, and as many pair of stockings, at the same time. Exclamations of surprise very frequently escaped from his friends at the rapid manner in which he increased and declined in corpulence.

NOTICE.

IN reply to the inquiry of a subscriber who wishes to procure the work now publishing under the direction of the Mass. Med. Society, we would state that the Society has no interest in any copies beyond those required for the present and perhaps future members. The publishers will probably strike off extra copies for sale, which may be procured, as published, of the principal booksellers in the United States. The work on Fever, will be published early in June; and if the plan is carried on, probably one or two numbers may appear each succeeding year.

Whole number of deaths in Boston the week ending April 8th, 25. Males, 10—Females, 15. Of consumption, 3—smallpox, 1—infantile, 3—old age, 2—croup, 1—dropsy, 1—inflammation on lungs, 3—scald, 1—unknown, 2—lung fever, 4—brain fever, 1—carbuncle, 1—dropsy on brain, 1—tumor, 1.

ADVERTISEMENTS.

COPARTNERSHIP NOTICE. JOHN COTTON and DAVID CLAPP, JR. have taken HENRY S. HULL into copartnership, and the printing and publishing business, which has heretofore been conducted at 184 Washington St. in the name of JOHN COTTON, will be continued under the firm of CLAPP & HULL.

D. C. and H. S. H. will give their personal attention to BOOK AND JOB PRINTING, of every variety, which will be neatly and correctly executed, upon reasonable terms.

*** Booksellers and authors are informed that the printing of medical and other works, of any size, will be undertaken as above, and every branch of their mechanical execution will receive particular attention. March 29.

THE NATURALIST. Devoted to Zoology, Botany and Mineralogy, illustrated by Lithographic Prints, edited by D. JAY BROWNE, aided by distinguished Naturalists, and published monthly by Peirce and Parker, No. 9 Cornhill, Boston. Each number contains 32 octavo pages. Price, Two Dollars a year in advance.

March 22.

VELPEAU'S MIDWIFERY. This day received and for sale by CARTER, HENDEE & BABCOCK—An Elementary Treatise on Midwifery, or Principles of Tokology and Embryology. By Alf. A. L. M. Velpeau, M.D. March 29.

NOTICE. Those gentlemen who are yet indebted for the 1st vol. of the Med. and Surg. Journal, from Feb., 1828, to Feb., 1829, are hereby notified, that, after four weeks, the Proprietors of that Volume have authorized Mr. S. Smith to collect all that is due to them for it; and the present Proprietor respectfully requests that those indebted to him for the Medical Intelligencer, and the 2d and 3d vols. of the Med. and Surg. Journal, will forward all arrears. JOHN COTTON.

The subscribers above mentioned for said 1st vol., are hereby requested to remit payment for the same to the subscriber, at the office of the Boston Daily Advertiser.

SAMUEL SMITH, Agent for said Proprietors.

March 1, 1831.

COPARTNERSHIP NOTICE. The subscribers have formed a connexion in business as CHEMISTS, DRUGGISTS & APOTHECARIES, at Apothecaries' Hall, No 188 Washington Street, opposite Marlboro' Hotel, under the firm of JARVIS & PEIRSON.

NATHAN JARVIS.
GEORGE W. PEIRSON.

EUROPEAN LEECHES.

J. & P. have a few fine European Leeches—to the application of which, when directed by Physicians, they will attend without any additional charge. Feb. 8.

WILLIAMS ON DISEASES OF THE CHEST. This day received, by CARTER & HENDEE, "A Rational Exposition of the Physical Signs of the Diseases of the Lungs and Pleura, illustrating their Pathology and facilitating their Diagnosis." By CHARLES J. B. WILLIAMS. Dec. 6.

BECLARD'S GENERAL ANATOMY. CARTER, HENDEE & BABCOCK have this day received—Elements of General Anatomy, or a Description of every kind of Organ composing the Human Body. By P. A. BECLARD, Professor of Anatomy of the Faculty of Medicine of Paris. Preceded by a critical and biographical Memoir of the Life and Writings of the Author. By OLIVIER, M.D. Translated from the French, with Notes. By JOSEPH TOGNO, M.D., Member of the Philadelphia Medical Society. Dec. 28.

The Boston Medical and Surgical Journal is published weekly, by CLAPP & HULL, at 184 Washington St. corner of Franklin St., to whom all communications must be addressed, *post-paid*.—Price three dollars per annum, if paid in advance, three dollars and a half if not paid within three months, and four dollars if not paid within the year. The postage for this is the same as for a newspaper.